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RESEARCH MEMORANDUM

ENLISTMENT SUPPLY INTO THE NAVAL RESERVE

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1. Enclosure (1) is forwarded as a matter of possible interest.

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ENLISTMENT SUPPLY INTO THE NAVAL RESERVE

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ABSTRACT

This research memorandum develops and estimates an econometric model of enlistments into the Naval Reserve. The analysis incorporates factors such as size of recruiting force, Navy veteran population, local unemployment rates, civilian wages, and recruiting goals.

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INTRODUCTION

Although many studies of military enlistment have been conducted during the last two decades, few have analyzed the reserve component.[1] Because of insufficient data, no studies have yet estimated the effects of recruiters or population on reserve enlistments, and the effects of pay and unemployment may be questionable due to the omission of key variables in the estimation. This memorandum presents estimates of enlistment supply into the Naval Reserve. It uses a newly constructed data base on monthly contracts for the 31 commands over a four-year period. The data are used to compare estimates from several econometric models.

The organizing principle behind the econometric specifications analyzed in this study is the need to deal with the correlation between the variables included in the estimating equation and the residual errors. Such a correlation may arise for a number of reasons, leading to different methods of correcting for it. If such a correlation exists, standard regression procedures will yield biased estimates of the parameters. The sources of correlation discussed in this memorandum are measurement error, unobservable effects, and the role of goals.

THE TRADITIONAL ENLISTMENT SUPPLY MODEL

Most analyses of enlistment supply have focused on the relationship between the number of enlistments, defined in a number of different ways, and exogenous variables such as pay, economic conditions, population size, and the number of recruiters. A common specification used is

$$Y_i = X_i\beta + \mu_i \,, \tag{1}$$

where Y_i is the number of enlistments in command or district i, X_i is a vector of explanatory variables, and μ_i is a random term assumed to be uncorrelated with X_i, with mean zero and variance σ_μ^2.

In recent years, this traditional model has been criticized as supplying biased estimates of the underlying parameters, raising doubt as to whether it is truly a supply equation that is being estimated. The principal deficiencies of this traditional model are the omission of the role of quotas, the effect of unobservable variables such as recruiter effort and local recruiting conditions, and the related econometric problem known as errors in variables. All of these problems are potentially serious and may lead to incorrect estimates and, therefore, inappropriate policy decisions if not correctly controlled for in the estimation procedure.

1. The analysis in [1] is the most comprehensive.

A more general formulation of the enlistment supply equation is

$$Y_{it} = \alpha_i + z_{it}\beta + \mu_{it} , \tag{2}$$

with α_i an unobserved command- or location-specific effect, and may be fixed or random. Furthermore, suppose that the true variable of interest z_{it} is unobserved or subject to measurement error so that what is observed is $X_{it} = z_{it} + \eta_{it}$, which in turn results in the following estimating equation for the observables:

$$Y_{it} = \bar{\alpha} + \beta X_{it} + \mu_{it} - \beta\eta_{it} + (\alpha_i - \bar{\alpha}) . \tag{3}$$

The basic problem with estimating equation 3 by OLS is the correlation between X_{it} and the composite error term. The asymptotic expected value of the OLS estimate of β is

$$\hat{\beta}_{LS} = \beta + \frac{Cov(\alpha_i, X_{it})}{\sigma_X^2 + \sigma_\eta^2} - \beta\frac{\sigma_\eta^2}{\sigma_X^2 + \sigma_\eta^2} . \tag{4}$$

Thus, this specification contains two sources of bias, the first due to the correlation between α_i and X_{it}, and the second attributable to the measurement error. Note also that the degree of bias due to mismeasurement increases with the variance of the measurement error σ_η^2.

Even in the absence of measurement error (i.e., $\sigma_\eta^2 = 0$), a nonzero covariance between X_{it} and μ_{it} may be expected. Suppose, for example, that recruiters within a command vary their effort according to the ease of meeting a goal. As a result, a systematic relationship will exist between recruiting effort, goals, and the underlying enlistment supply function. Because effort is not observable, its impact will be partly reflected in the error term μ_{it}, but since effort is also correlated with true supply, μ_{it} will be correlated with the included variables, and estimates of β will be biased.

Even in the absence of any variation in effort, there may be correlation among goals, recruiters, and the error term because of simultaneity in assigning goals and recruiters. For example, if goals are set to provide the proper incentives to recruiters, they will be related to local supply conditions such as the population size and the number of recruiters (for goals at the command level). But in that case, goals and enlistments will be affected by many, if not all, of the same factors to be estimated in the supply equation. In effect, goals are set by supply conditions, but enlistments may in turn be affected by goals, so that the two variables are determined jointly. If what is purportedly a supply function is naively estimated, how can it be known that it is not a goal-setting function that is estimated? The possibility is not so far-fetched as it might at first appear. For example, if goals were always met exactly, no more and no less, it is highly likely

that observed enlistments do not represent supply but rather goals, and a goal-setting equation will be estimated. Empirical research must evaluate this possibility carefully if supply models are to be used for anything besides prediction. The situation with regards to simultaneity is analogous in the assignment of recruiters to the local commands.

The specific method chosen to solve a particular problem or set of problems will depend to a large extent on the ultimate goal of the analysis. There are typically two goals of enlistment supply analysis. The first objective is to develop the ability to predict future enlistments with some accuracy and forewarning, so that the appropriate policy plans can be implemented. The second goal is to estimate the underlying, or "true," enlistment supply curve. The two objectives do not always coincide, because prediction does not necessarily require the same information about supply conditions. For example, if prediction were the sole objective of the analysis, the role of goals or quotas is not as critical as long as the goals are relatively stable. If goals and the structural supply relationship interact in a systematic fashion and goals do not vary much, it is not necessary to separate the individual effects of goals and supply to generate accurate forecasts.

If, however, the interest is in understanding the structural supply relationship, it will be necessary to distinguish between the intertwined effects of goals and enlistments, as well as any complex interdependencies with other variables. Even if prediction accuracy were the sole criterion, it would be necessary to specify the impact of goals if they are expected to change significantly in the future. This paper focuses on recovering the underlying structural supply relationship and not solely on prediction accuracy. In addition to its intrinsic empirical interest, the emphasis on the supply relationship is considered necessary for enlistment supply analysis of the Naval Reserve, since the goals have changed significantly during the last several years, and will probably continue to change in the future.

Reference [2] describes in greater detail the statistical problems of estimating enlistment supply. This memorandum presents some estimates of various models in an effort to identify the best approach to the problem.

EMPIRICAL RESULTS

Construction of the Data Set

The recruiting data used in this analysis come from the RESULTS module of the Reserve Training Support System (RTSS). The raw data files contain information on each recruit, including social security number, rating, date of enlistment, and location. It also contains the SSN of the recruiter and the command and attainment unit identification codes for the units receiving credit for the enlistment. The individual records were aggregated by command and date of enlistment to create a

file from October 1982 to September 1986. (For the monthly state enlistment file, the data were aggregated according to the state the attainment unit was in.) Because the Sea and Air Mariner (SAM) recruiting data on the RESULTS system are incomplete, data from PRIDE-R were merged with the RESULTS data to create a complete reserve recruiting data base. For SAMs, the contract date was used as the date when the recruiter received credit for the enlistment, even though the actual shipping date may not occur for months.

The number of recruiters in each command was calculated from the RESULTS data as the number of recruiters in each month that had at least one enlistment credited to them. Because recruiters will appear on the file only if they enlist someone, this method means that some under-counting will occur if recruiters are on board and working but do not sign any recruits. This does occur occasionally, especially for new recruiters, but there is no reason to expect that it is systematically related to either the command or time period; therefore, the estimates should not be biased to any significant degree.

Monthly state unemployment rates were collected from the monthly issues of *Employment and Earnings*, a publication of the Bureau of Labor Statistics (BLS). The unemployment rate used is the one for all workers and is not seasonally adjusted. Civilian earnings data were also collected from the BLS; the quarterly Employment Cost Index (ECI) for wages and salaries for four census regions was used as an estimate of the civilian wages earned by enlistees. The index of military pay used in the analysis is the average annual earnings of an E-4 with four years of service. For the statistical analysis, the relative civilian:military pay ratio is used.

This estimate of civilian earnings opportunities has several problems. First, because only four census regions are available, the level of aggregation masks some of the differences in wage levels among states, as well as variations within states. Second, the use of average regionally based estimates of wages confounds the estimation of true pay effects with unobserved regional preferences. As a result, the estimates of pay effects are likely to be unstable or even perverse. This problem is exacerbated by the special circumstances of reserve affiliation, since the issue is not a well-defined occupational choice problem, as it is with active-duty enlistments, but more a matter of supplementing income or a desire to do something different on a weekend. To understand the effect of civilian wages on reserve affiliation requires the actual earnings of the target population. Without such data, the available estimates are questionable. Within broad geographic regions, there is too much wage variability to depend on averages to get reliable estimates. Because of these deficiencies, the pay estimates are not expected to be reliable.

Similar problems pertain to unemployment rates as well. Although these figures may do a good job of measuring broad economic opportunities, they are not likely to measure some demographic groups as effectively, and are a particularly poor measure for groups with good job skills, such as trained Navy veterans. A more fruitful area to investigate is the post-service employment histories of recently separated veterans. If available, such data show whether the affiliations are from those who remain unemployed for a time and join the reserves for some extra cash and something to do or whether they are already established in a civilian job. This type of information is crucial for understanding the affiliation decisions and, subsequently, retention decisions.

Although the Selected Reserve includes non-prior-service personnel as well as veterans from other services, most enlistments are recently separated Navy veterans. Rather than attempt to deal with the small-number problems, enlistments from all categories except SAMs were aggregated. The population estimates used in the analysis are a 12-month moving sum of Navy separations calculated from the Enlisted Master Record (EMR), allocated to states on the basis of the state of record listed on the EMR on the date of separation. Although the state of record is not always accurate, no alternative data source with similar coverage and greater accuracy was identified.

Recruiting goals for the 31 commands were obtained from COMNAVRESFOR, Code 922. Because the only available goals were the annual fiscal year objectives, monthly goals were estimated as the fair share of the annual goal. Published monthly goals for FY 1987 demonstrate that the annual goals are in fact allocated on a level-loading basis without regard to varying supply conditions during the year. Some inaccuracy may result from the use of the end-of-fiscal-year goals, if they differ from the goals used during most of the year. Goals were changed in FY 1986 during the course of the year, but it is unknown whether similar changes occurred in earlier years.

Although most of the variables used in this analysis have been used previously in studies of active-duty enlistments, it is highly likely that many of the variables will not have the same impact as found in earlier work. The principal reason for this is that the nature of the reserve obligation differs significantly from that of the typical active-duty enlistment. The reservist is signing up for part-time work (one weekend a month plus two weeks of active duty per year), whereas, in most cases, the active recruit signs a contract for at least four years. For reservists, the question is not really an occupational choice problem, but more a decision regarding an income supplement and part-time work. Therefore, the effects of military and civilian earnings and unemployment will not necessarily have the same impact.

Geographic Differences in Enlistment Supply

Table 1 presents a list of states by divisions. Table 2 summarizes the variations in average monthly contracts per recruiter for the nine census regions. Two conclusions are immediately obvious. First, all nine regions show a significant drop in average recruiter productivity during FY 1984, which is probably due to the introduction of the SAM program and a shift in emphasis away from recruiting other supply sources. Second, the western regions (regions 8 and 9) persistently show higher productivity than the other regions, and the northern regions (1, 2, and 3) do the worst. The West South-Central region, which includes the petroleum-producing states of Texas and Louisiana, became much more productive. It is noteworthy that an identical pattern can be found in active-duty recruiting, indicating perhaps that there are systematic differences in regional preferences for military service, or that economic conditions have a similar impact on recruiting for the two components.

Tables 3 and 4 show some of the reasons for the differences in productivity across regions and over time. The unemployment rate was high in 1983, which helped make recruiters productive. Since then, unemployment rates have fallen steadily, which would be expected to reduce enlistments. The unemployment rate is lowest in the Northeast and highest in the Midwest; however, the East North-Central region (region 3) has experienced the largest decrease in the unemployment rate, which dropped from 13.0 in 1983 to 8.3 in 1986. Both the Northeast and North Central regions continued to have decreases in the unemployment rate from 1985 to 1986, the West saw only a minimal improvement, and rates in the southern regions actually rose in some areas.

Another important factor in the productivity of recruiters is the relative decline in real earnings of workers in the West and the steady rise in earnings for employees in the Northeast. Wages in the South and Midwest have moved more erratically, with some upward trend. Combined with the unemployment figures, these results provide some explanation for the differing productivity around the country.

TABLE 1

LIST OF STATES BY DIVISION

REGION
Divisions
State

NORTHEAST

1 New England

Connecticut
Maine
Massachusetts
New Hampshire
Rhode Island
Vermont

2 Middle Atlantic

New Jersey
New York
Pennsylvania

NORTH CENTRAL

3 East North-Central

Illinois
Indiana
Michigan
Ohio
Wisconsin

4 West North-Central

Iowa
Kansas
Minnesota
Missouri
Nebraska
North Dakota
South Dakota

SOUTH

5 South Atlantic

Delaware
District of Columbia
Florida
Georgia
Maryland
North Carolina
South Carolina
Virginia
West Virginia

6 East South-Central

Alabama
Kentucky
Mississippi
Tennessee

7 West South-Central

Arkansas
Louisiana
Oklahoma
Texas

WEST

8 Mountain

Arizona
Colorado
Idaho
Montana
Nevada
New Mexico
Utah
Wyoming

9 Pacific

Alaska
California
Hawaii
Oregon
Washington

TABLE 2

AVERAGE MONTHLY ENLISTMENTS PER RECRUITER

Region	Fiscal year 1983	1984	1985	1986	All years
1	5.2	3.3	3.6	3.7	3.9
2	4.8	3.2	3.6	3.9	3.8
3	4.6	3.0	4.0	3.7	3.8
4	4.8	3.3	3.7	4.0	3.9
5	4.9	3.5	4.1	4.5	4.2
6	4.3	3.2	4.3	4.3	4.0
7	4.1	2.9	3.8	4.2	3.8
8	4.7	4.0	4.3	4.2	4.3
9	5.7	3.9	4.0	4.7	4.5
All	4.9	3.4	3.9	4.2	4.0

TABLE 3

UNEMPLOYMENT RATES

Region	Fiscal year 1983	1984	1985	1986	All years
1	7.4	5.4	4.4	4.1	5.3
2	10.0	7.9	7.0	6.5	7.8
3	13.0	9.9	9.3	8.3	10.0
4	8.2	6.6	6.2	6.1	6.8
5	8.9	6.8	6.4	5.9	6.9
6	12.4	10.3	9.2	9.2	10.2
7	8.9	7.3	7.6	9.0	8.2
8	9.4	6.5	6.7	7.3	7.5
9	10.5	8.5	7.7	7.2	8.5
All	10.1	7.9	7.4	7.1	8.1

TABLE 4

AVERAGE WEEKLY EARNINGS (DOLLARS)

Region	Fiscal year 1983	1984	1985	1986	All years
1	202	208	210	215	209
2	222	227	226	227	226
3	263	274	271	271	270
4	222	225	221	224	223
5	186	191	191	195	191
6	187	192	195	199	194
7	219	224	221	223	222
8	217	218	216	216	216
9	243	241	232	232	237
All	223	227	225	226	225

Further evidence of the importance of geographic differences in enlistment supply is provided by examining the relative rankings of regions or recruiting commands over time. Table 5 shows the rank order correlation of average recruiter productivity by command over different time periods. The correlations are consistently high and positive, showing that there are systematic differences among recruiting commands in productivity and that these differences persist over a period of years. As would be expected, the correlations are highest in consecutive years, but they are also large over a three-year interval, as indicated by the correlation of 0.36 between 1983 and 1986. Even this estimate is likely to be too low, as the table shows a pattern of rising correlation in recent years. (The correlation between 1983 and 1984 is 0.55, whereas the rank order correlations between 1984 and 1985, and 1985 and 1986, are 0.67 and 0.79, respectively.)

TABLE 5

RANK ORDER CORRELATION OF
AVERAGE RECRUITER PRODUCTIVITY
BY COMMAND

Year	1983	1984	1985	1986
1983	1.00	0.55	0.50	0.36
1984	--	1.00	0.67	0.55
1985	--	--	1.00	0.79

A similar pattern can be seen in table 6, which gives the correlation between average productivity in different years using command data. All the tables indicate that 1983 was a peculiar year, with correlations much lower than in succeeding years. This may be due to the impact of the SAM program, which may have had the effect of altering the distribution of productivity among commands, although why this would occur is unknown.

TABLE 6

CORRELATION OF AVERAGE PRODUCTS
BY COMMAND

Year	1983	1984	1985	1986
1983	1.00	0.38	0.59	0.50
1984	--	1.00	0.53	0.54
1985	--	--	1.00	0.79

Although not a substitute for a formal statistical analysis, this discussion has identified some factors that are likely to be important in the analysis of enlistment supply. First, changes in unemployment rates and real earnings appear to affect recruiter productivity. Second, the introduction of the SAM program may have caused a sharp reduction in the number of enlistments. These effects will be controlled for in the statistical analysis.

Regression Results

The sample statistics are provided in table 7. Table 8 shows the coefficient estimates for the OLS, fixed-effects, and random-effects models. As is evident from casual inspection, the estimates are sensitive to the specification used, and some of the results can be described only as perverse. For example, the OLS estimate of the effect of unemployment rates is negative, i.e., higher unemployment will reduce enlistments. The estimate is both large and highly significant. Some clue as to what is going on can be seen by looking at the fixed-effects estimate, which is no longer negative or significant. Because the fixed-effects estimate looks only at variation within commands, this implies that, for some reason, regions with above-average unemployment rates tend to have lower than average enlistments.

The pay effect is also bizarre, with a large positive elasticity in the fixed-effects model, implying that higher civilian wages increase enlistments, which is counterintuitive and at odds with most previous estimates. The population elasticity is positive in all specifications, but the fixed-effects estimate is six times larger than the OLS coefficient. Recruiting goal is significant only in the OLS equation. The recruiter elasticity is a bit more stable, ranging from 0.54 to 0.64. Other results of interest are the small, though significant, effect of SAM recruiting and some sizeable seasonal variation in enlistments (not shown in the table).

TABLE 7

SAMPLE STATISTICS[a]

	Mean	Standard deviation
Enlistments	58.8	39.8
Recruiters	13.6	8.8
Unemployment rate	8.1	2.2
Population	5,928	3,157
Civilian/military pay	0.744	0.04
SAM recruits	22.8	26.3
Goal	711.8	443.1

a. Data on 1,440 observations from 30 commands during October 1982 through September 1986.

TABLE 8

REGRESSION RESULTS[a]

	OLS	Random	Fixed
Log (recruiters)	0.638 (19.3)	0.564 (17.1)	0.542 (16.0)
Unemployment rate	-0.026 (6.2)	-0.009 (1.5)	0.007 (1.0)
Log (population)	0.142 (7.9)	0.289 (5.7)	0.848 (5.4)
Log (goal)	0.282 (7.7)	0.209 (4.8)	0.031 (0.6)
Log (relative pay)	-0.179 (1.2)	0.412 (0.9)	4.163 (3.5)
Log (SAMs)	-0.023 (10.3)	-0.019 (8.0)	-0.019 (7.4)
R-square	0.8694	0.5060	0.9014

a. Each specification contains dummy variables for each month and 1,440 observations.

DISCUSSION

As a result of the foregoing discussion, three points should be made:

- Location- or command-specific effects are correlated with pay, unemployment, and population.[1]
- The perverse pay effects may be evidence of measurement error in the pay variable.
- Although goals are significant in annual data and even the OLS for monthly data, it is not clear that they play a role after command effects are controlled for.

1. This is confirmed by a Hausman specification test [3], which rejects the validity of the GLS specification.

In deciding what should be done, one option is to accept the fixed-effects estimates and attribute the pay elasticity to the peculiarities of reserve institutions and obligations. More fruitfully, perhaps, is to investigate some alternative models, given the evidence that something unusual is occurring. The large differences in the estimated coefficients indicate that a simple model is inadequate for the behavior of reserve enlistments. One approach, as outlined in [2], is to allow for errors in measurement for some of the key variables, or, alternatively, view some of the included variables as correlated with unobservables. The latter is likely to be a better solution than accepting the fixed-effects results, since they will be tainted if there is any measurement error.[1]

Another deficiency of the fixed-effects model is more subtle. The model assumes that the sources of differences in productivity among commands are fixed over the sample period. (This is also true for the random-effects model.) This assumption is not likely to be valid when the factors that may actually be causing the differences are considered. One possibility is that there are unmeasured variations in the employment opportunities available to potential reservists. Another factor may be that the recruiters in a particular command may be more capable or the commanding officer may be a better motivator. None of these factors is likely to be constant over a four-year span. Relative economic conditions are constantly fluctuating, so that civilian opportunities are changing. Commanding officers are rotated every few years, and the recruiting force turns over as well. Consequently, differences among commands are not likely to be fixed and immutable over time, but will constantly change. Hence, the dummy-variable approach will not eliminate the correlation between the error term and the left-out variables but will more likely aggravate the problem of correlation. In sum, the problem is that the "fixed effects" are not fixed at all.

The introduction of measurement error and unobservables into the analysis leads to the use of instrumental variable techniques [4]. (The appendix discusses instrumental variable estimation in more detail.) Recent years have seen extensive development in the econometric literature for dealing with problems of unobservables or latent variables, including mismeasured ones.[2] The solution in this case is to introduce some new variables that are correlated with the latent variables but not the errors. Although, in most contexts, this is difficult to do, some options are available in the recruiting analysis. If, for example, the number of recruiters in a command is thought to be correlated with the unobservable, it may be reasonable to use the total number of recruiters, plus the other exogenous variables, as instruments for the

1. Similar reasoning is found in [3] in its discussion of the analysis of panel data.
2. See, for example, [5], [6], and [7].

number of recruiters in a command. Similarly, it seems plausible to consider that the level of effort in a command will depend on the size of the goal, so that the goal will also be correlated with unobserved effort. As mentioned earlier, available measures of civilian earnings are not good and are likely to give biased estimates of the effect of pay.

For the reasons already discussed, the variables that are expected to be correlated with the error terms are recruiters, goals, and pay. To achieve identification of β, some variables are needed that are correlated with recruiters, pay, and goals but are not all in the enlistment supply equation. It is logical to view this problem as one of allocating national resources to local commands. That is, the goal for all enlistments is set, then allocated to the local commands based on factors such as their perceived ability to meet them and equity concerns. Similarly, the total number of recruiters available depends on fiscal and manpower constraints, which are operative at the national level. Once the total number is set, goals are allocated to the local commands. Then feasible external instruments for the goal and recruiters at the local command will be the national enlistment goal and the aggregate number of recruiters. Hence, there are sufficient variables to identify the parameters of interest. The aggregate totals should not be correlated with the errors at the local level, since the decision process generating those numbers is independent of the local conditions. Therefore, both the national goal and total number of recruiters satisfy the requirements to be valid instruments. For the pay variable, the national ECI is used as an additional variable.

Estimates of the instrumental variable model are provided in table 9. The recruiter elasticity is about the same as the fixed-effects estimate--0.53 versus 0.54; the population estimate drops by about half; and the pay elasticity now has the right sign, although it is only marginally significant. If measurement errors in the pay variable are allowed for, the pay effect increases in magnitude and is now significant at the 10-percent level. The unemployment rate, however, has a statistically significant negative coefficient, even larger in absolute value than the original OLS estimate. So although the pay variable seems to have improved, the unemployment effect is counter-intuitive. Although the unemployment rate is likely to be mismeasured,[1] attempts to instrument the local unemployment rate with the national rate did not succeed.

1. It is constructed as an average of the states the commands are in, so it is really only a rough approximation of the true opportunity.

TABLE 9

INSTRUMENTAL VARIABLE ESTIMATES[a]

	I	II
Log (recruiters)	0.533 (5.1)	0.531 (5.1)
Unemployment rate	-0.032 (5.7)	-0.032 (5.7)
Log (population)	0.455 (4.7)	0.456 (4.7)
Log (goal)	0.010 (0.1)	0.014 (0.1)
Log (relative pay)	-0.286 (1.5)	-0.354 (1.9)
Log (SAMs)	-0.018 (5.6)	-0.018 (5.6)

a. All specifications contain 1,440 observations. Model I treats enlistments, recruiters, and goals as endogenous. Model II uses an instrumental variable for relative pay. The absolute value of t-statistics is in parentheses.

So far, there is still a range of estimates with no good basis on which to judge them, but the data provide a roundabout way of checking the plausibility of the estimates. The sample from which the data come is based on the RESULTS module of RTSS and consists of all the individual recruits enlisting in SELRES from 1983 through 1986. For the purposes of this analysis, the individual records were aggregated into commands, primarily to deal with the issue of goals and also because that is the administrative structure of the recruiting apparatus. For analytical purposes, however, the raw data can be aggregated in some other way, so as to create, in effect, a new sample that may not suffer from the deficiencies of the command-level data. In particular, aggregating the data by state provides better estimates of the relevant unemployment rates and, at the same time, breaks down (although it does not eliminate) the close connection between enlistments, quotas, and the assignment of recruiters. Estimating the model on state-level data provides a chance to adjust the sample and create a new data set by a not quite random reassignment of recruiters to locations.

Table 10 lists the results of the estimation of monthly state enlistments. The results, allowing for correlations with the error term, generate plausible coefficients in all variables. Both the recruiter and population elasticities are smaller than in table 9 but well within the bounds of expectations and highly significant. The pay variable is also highly significant and a much healthier -0.82. The unemployment rate is now positive and a reasonable size, 0.03; so the results tend to confirm the results of the simultaneous estimates and explain the "funny" unemployment-rate estimates.

TABLE 10

STATE REGRESSIONS[a]

	OLS	FIXED	IV-I	IV-II
Log (recruiters)	1.037 (62.5)	0.689 (19.8)	0.447 (4.4)	0.437 (4.2)
Unemployment rate	0.000 (0.1)	0.034 (5.9)	0.030 (4.6)	0.030 (4.5)
Log (population)	-0.016 (1.2)	0.805 (6.2)	0.304 (5.3)	0.319 (5.4)
Log (relative pay)	-0.124 (0.8)	3.056 (2.9)	-0.296 (1.3)	-0.821 (3.3)
Log (SAMs)	-0.025 (4.0)	-0.026 (3.8)	-0.026 (2.2)	-0.027 (2.2)
Log (national goal)	-0.230 (3.5)	-0.153 (2.2)	0.087 (0.8)	0.103 (0.9)

a. All specifications contain 1,248 observations. Model IV-I treats enlistments, recruiters, and goals as endogenous. Model IV-II in addition uses an instrumental variable for relative pay. The absolute value of t-statistics is in parentheses.

The instrumental variable approach just described can be given an alternative interpretation. Instead of thinking of recruiters, goals, and pay as correlated with some undefined unobservable, they can be viewed as part of a standard simultaneous equation problem. In this perspective, the process of allocating recruiters and goals is not (or at least should not be) independent of the supply factors. Instead, recruiters and goals are assigned as part of a rational process, perhaps

as an effort to maximize total enlistments or to address other considerations such as equity. The OLS and fixed-effects estimates would still be expected to be biased for the usual simultaneous equation reasons. The implementation of the solution is the same as shown above in the instrumental variable estimates, although the interpretation may be slightly different.

Several lessons are to be learned from this exploration of different enlistment supply specifications. First, the results are sensitive to estimation method, and some thinking has to precede the estimation process. Second, there is strong evidence of both simultaneity and measurement error in the data, and, without taking them into account, the results will be highly unreliable. Third, it is unclear whether the results of this analysis using reserve data will carry over completely to other services. Peculiarities of the reserve establishment may have caused greater problems in the data, but the expectation is that the qualitative nature of these findings will not be much different if applied to other samples. Last, better estimates of civilian earnings and employment prospects are absolutely crucial to obtaining an accurate answer to the question of what impact civilian pay and unemployment have on enlistment supply.

CONCLUSIONS

This paper has presented a variety of estimates of enlistment supply models for Naval Reserve affiliations. Although many of the estimates do not agree, several conclusions can be drawn from the analysis. First, the recruiter elasticity can be reliably estimated to be about 0.44 to 0.65, with a preferred estimate of 0.53. This means that a 10-percent increase in the number of recruiters is expected to raise enlistments by 5.3 percent. Because the range of estimates is relatively small despite the differences in econometric specification, manpower planners can use the results from this part of the study with some confidence in dealing with prospective policy changes.

A second finding is that the impact of recruiting goals at the command level is negligible. In other words, there is no strong evidence to support the proposition that enlistments can be increased solely by increasing goals, as has been found in some other studies [8,9].

This analysis also found evidence of a significant effect of the size of the population of recently separated Navy veterans, with the most reliable elasticity estimates ranging from 0.32 to 0.85. Although the range is large (even when the implausible OLS estimates are ignored) due to the variety of specifications tested, the results should be useful in projecting future NAVET affiliations based on expected separations from active duty.

The principal problem areas identified in this study were the economic variables of pay and unemployment. The pay estimates range from 4.16 to a more reasonable -0.82, but the results are in such disagreement that doubt must be cast on all the estimates. The only good solution to this problem is to develop much better estimates of the expected earnings of the target population.

Although the coefficient on the unemployment rate was sensible in using state data, the curious findings using most of the other specifications show the need for further modeling development. The results for the fixed-effects model seem to show that the source of the problem is the correlation between the unemployment rate and unknown location-specific factors, but at this time, it is difficult to make any general conclusion with confidence.

An indication of where to improve the pay estimates can be seen in figure 1, which shows indexes of average earnings for different age groups. The graph demonstrates that aggregate measures of civilian pay, such as the Employment Cost Index, do not reflect the earnings of younger workers, especially during the last several years. Consequently, it is not surprising that the pay coefficients estimated in this study cover such a wide range and are so implausible. A high priority for future research should be the construction of an accurate index of civilian earnings for the population that is the target of recruiting.

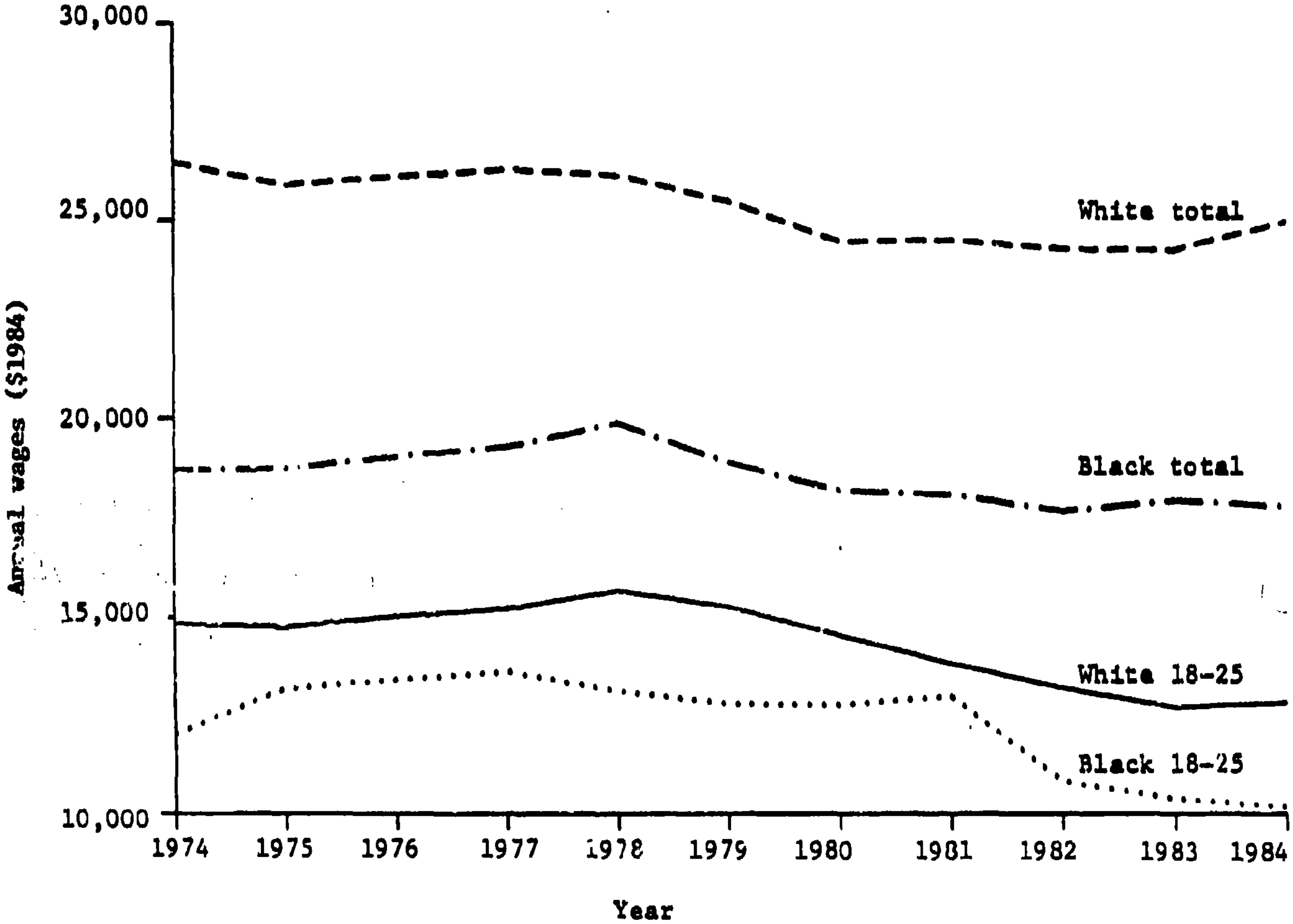

FIG. 1: ANNUAL EARNINGS BY AGE GROUP,
MALE FULL-TIME WORKERS

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1. The number in parentheses is an internal CNA control number.

APPENDIX

INSTRUMENTAL VARIABLE ESTIMATION

APPENDIX

INSTRUMENTAL VARIABLE ESTIMATION

The use of the instrumental variable estimator can be motivated as follows. Let the model be

$$Y = X\beta + u \,. \tag{A-1}$$

If X and u are asymptotically uncorrelated we can write

$$\frac{1}{n} X'Y = \frac{1}{n} X'X\beta + \frac{1}{n} X'u \,, \tag{A-2}$$

where equation 4 of the main text is multiplied by $(1/n)X'$. With uncorrelated X and u, $(1/n)X'u$ goes to zero in probability, and $(1/n)X'Y$ and $(1/n)X'X\beta$ will have the same limiting distribution, so that the logical choice of estimator is $\hat{\beta} = (X'X)^{-1}X'Y$, the OLS estimator. If, however, $(1/n)X'u$ does not tend in probability to zero, the OLS estimator will be inconsistent.

A solution to this problem exists if there is matrix Z, with the properties $(1/n)Z'X \overset{P}{\to} 0$, and $(1/n)Z'X$ tends in probability to a nonsingular matrix of constants. Then

$$\frac{1}{n} Z'Y = \frac{1}{n} Z'X\beta + \frac{1}{n} Z'u \,, \tag{A-3}$$

but this time, $\frac{1}{n} Z'u \overset{P}{\to} 0$, and the instrumental variable estimator

$$\tilde{\beta} = (Z'X)^{-1}Z'Y \tag{A-4}$$

is consistent.